Evaluation of the importance of typical nursery grounds of Sacramento splittail with otolith fingerprinting techniques

Guohua Zhang

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0114: Evaluation of the importance of typical nursery grounds of Sacramento splittail with otolith fingerprinting techniques

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This proposal represents an increasingly popular enterprise to reconstruct classes of juvenile habitat use from otolith microconstituents. Here the PI s propose to classify three fundamental juvenile habitats (Yolo Bypass, Sutter Bypass, and Suisun Bay marshes) of splittail, a species of concern (threatened in their proposal). Through such classifications, otoliths of adults can be classified in terms of which of these habitats contributed to recruitment. This approach for estimating "connectivity" has been featured in the recent literature for identifying essential fish habitat (see Beck et al. Bioscience article). By understanding relative contributions of nurseries on an annual basis, better links between watershed management practices and population dynamics of splittail could be established. PI s will rely on samples drawn from nursery regions from other research/monitoring programs and measure otolith microconstituents in the most recently formed portion of the otolith. Preliminary experiments showed interesting dose response in Se exposure studies and significant separation of fish from differing nursery systems. Through multiple year and system sampling, the PI s wish to develop a "fingerprint" library that would allow classification of adults to nursery systems across seasons and years.

Additional Comments:

There is inadequate justification that these habitats represent representative nurseries, that the sampling design can characterize within nursery variances, that variances in elemental fingerprints can be judged suitably stable within and among seasons to be useful, and that ontogenetic effects due to physiology alone can be disentangled from those effects of exposure to differing habitats. The PI s are new to the field and in preliminary work have not provided particularly compelling evidence that they can successfully carry out the investigation. There is evidence that the PI s will make mistake given the large suite of elements they intend to investigate and likely contamination, which will result from their otolith dissection and preparation procedures. No use of Certified Reference Material and established otolith trace element standard operating procedures are described. There was concern about the ability to unambiguously assign adults to nursery habitats given the rather limited spatial coverage. The budget for this type of study is fully unjustifiably high based upon sample sizes and likely research products. Although two reviewers gave good comments, their comments were in fact much more critical. All reviewers favored the general approach of using otolith chemistry to infer past contributions of nursery systems to recruitment, but this was more a general endorsement of approach, rather than support of this particular application. Two reviewers stated that there was evidence that the PI s were promoting a technique looking for a problem. Criticisms were inadequate attention to sampling design, a padded budget, lack of sufficient expertise, particularly in fish ecology and otolith chemistry studies, poor experimental protocols, and a poorly prepared proposal.

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Evaluation of the importance of typical nursery grounds of Sacramento splittail with otolith fingerprinting techniques

Otolith chemistry is very difficult with large potential for error. PI’s are new to field and demonstrate no experience in the field. The experimental design was considered poor. There was no use of certified reference materials. The budget was considered fully unjustified. The work proposed to address the temporal and spatial pattern of "fingerprints", one of the more positive aspects of the proposal.

Final Ranking: Inadequate

Technical Review #1

proposal title: Evaluation of the importance of typical nursery grounds of Sacramento splittail with otolith fingerprinting techniques

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals are overstated given the lack of current knowledege about splittail population dynamics and the background of the PIs. The hypotheses are mostly a list of questions restated and not posed in a way that show the PIs have a clearly defined plan to attack this problem.**
Rating	**fair**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The overall feeling I get from the proposal is that it is a technique looking for a problem. Although this is not a total condemnation of the PIs approach because I think that otolith analysis is a useful tool to gathering information about these questions. My concern is based on the lack of a connection between all the otolith analysis and the overall biological problem. Why is it important to determine the contribution of juveniles to the spawning population? This is one of a number of biological questions that needed to be integrated into the proposal to strengthen the justification for the use of otolith**

	analysis over more traditional techniques, such a mark and recapture.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Too heavily skewed toward the otolith analysis and very little mention of the interpretation of the data.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Given the approach and the focus of the proposal I do not think the results of this work will achieve the stated objectives.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Statements about interpretation of the otolith data are very general and vague. Not clear who would be responsible for this portion of the work or if they are capable of doing so.
Rating	poor

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The establishment of an otolith library is a good thing and would be a worthwhile product of this work.**
Rating	**good**

Additional Comments

Comments	**The writing is somewhat sloppy and vague. Minor but numerous grammatical errors were a source of some frustration to this reader. It gave me the impression that the proposal was prepared in haste and not thoroughly proof read.** **The splittail is not currently classified as "Threatened" but is a "Species of Concern". This distinction is more than semantics and further reinforced my impression that the proposal was put together at the last minute.** **Letters of support from Moyle and Sommer mention a project with a different title.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**PI's have a good track record in their work on endocrine disruptors and heavy metal toxicity. They are currently well funded in these areas. Unfortunately, they have no publication record on otolith analysis or use in fisheries applications. The co-PI has a PhD dissertation on this subject but has not published this work. I am concerned that their**

	lack of direct experience in this field will lead them to get in over their heads and not be able to accomplish their objectives.
Rating	fair

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This is labor−intensive work and the budget relects that effort. There is a fair portion of the budget dedicated to equipment rental. I am assuming this is for the actual otolith analysis but it has not been specified.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	Many inconsistencies in the proposal and an overall lack of a strong justification for this approach. My main concern is the lack of documented experience by the PIs in the interpretation of otolith analysis. The inclusion of a fisheries biologist would have strengthened their claims that the otolith analysis could be transformed into a tool to manage the splittail populations.
Rating	fair

Technical Review #2

proposal title: Evaluation of the importance of typical nursery grounds of Sacramento splittail with otolith fingerprinting techniques

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The researchers propose to generate elemental and isotopic "fingerprints" in the otoliths of juvenile splittail from different potential juvenile nursery areas and then use these groundtruthed fingerprints to examine the contribution of these nursery habitats to spawning populations. Overall, I found that the overall goals of the project are both timely and important. The use of otolith chemistry to answer such questions is becoming increasingly common, especially in estuarine and freshwater systems where we might expect significant variability in water chemistry.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**I found that the study is certainly well justified given the importance of determining essential habitat used by splittail in this system. I agree with the authors that approach that they suggest is likely to be very cost−effective given the other alternatives (mark−recapture, for instance). If successful, I would imagine that the information provided by this study**

	will be extremely useful in establishing areas for restoration and evaluating the results of these restoration efforts.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The general approach outlined here is appropriate for meeting the project objectives. The research outlined here is certainly feasible. I think that the authors have made a sound decision to include carbon and oxygen isotopes in these analyses. I would have included Sr isotopes as well. The project uses methodologies that are, at this stage, well established. Laser ablation ICP−MS is becoming widely accepted in otolith studies, and carbon and oxygen isotope analysis using isotope ratio mass spectrometry has been well established for over a decade. So, why the authors are unlikely to provide novel methodologies the chances of at least some success if high because the research proposed here doesn't require the development of new analytical approaches. Ultimately, if successful, I believe that the information will be extremely useful for decision makers as it may be able to both identify important splittail habitats and potentially could be used to evaluate the effectiveness of restoration sites.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**I found that the overall approach outlined here is well documented and (generally) technically feasible. There are, however, several points that left me a litle uncomfortable with the familiarity of the authors with these tyes of analyses. For instance, the list of elements that the authors give on page 5 (a total of 18 elements to be analyzed by ICP−MS) is clearly unrealistic by any standards. They seem to have got this list from early papers in the field that are now widely regarded as optimistic (to be kind). Realistically the authors are going to be dealing with 5−6 elements. With that in mind, I was disappointed that we weren't given more information on the data presented in Figs. 2 and 3. Which elements were above detection limits (and how was this defined?). I find it quite unlikely that the authors were able to detect Fe and Co in the splittail otoliths. And because of complicated interferences on both these elements they wouldn't know if the data were good with using a high resolution ICP−MS at least initially. The fact that these elements generated the differences among locations is worrying.** **Second, I was a little concerned with the authors ability to unambiguously assign adults to nursery habitats given the rather limited spatial coverage (two sites in the floodplan, two in the estuary, and another 3 in river systems). The problem is may there be areas that the authors didn't sample that have overlapping fingerprints with the sites selected here? Might we expect that there are unambiguous isotopic or elemental fingerprints of residence in any of these 3 location types? I am not sure that we know the answer yet.**
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The project here doesn't depend upon a BACI-type experimental design. However, this is an opportunity to note that there is very little in the terms of statistical approaches that will be used to classify unknown fish using the isotopic and elemental fingerprints.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Overall, I think that if successful the project would undoubtedly make a significant and valuable contribution to the overall program.**
Rating	**very good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The authors have got a good track record in their fields. Dr. Teh is a toxicologist with a solid publication record, and Dr. Zhang is a promising post-doctoral researcher. However, neither of the authors have much experience with otolith chemistry which is quite clear in the proposal. For instance, they suggest that a number of elements (Sr, Ba, Li, Cu, Hg, and Pb) are deposited in proportion to their concentration in ambient water. In fact this is a quite hotly debated subject currently in the field**

	that has suffered from poor experimental approaches in the most of the studies that the authors list. **Realistically, the authors will be very dependent on the expertise of the staff that run the ICP-MS facility at UC Davis for appropriate analytical techniques and interpretations. I am sure that they are very good, and certainly the facilities themselves are excellent.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget for this work, at a total of 758k, is significant. A good amount of these funds are salaries, and these would seem to be reasonable given the realities of soft-money funding. However, I was a little taken aback by the 160k that is budgeted for the isotopic and elemental analyses. For instance, in terms of ICP-MS, these funds would provide a total of over 200 days of instrument time. If we say that 1/2 of these funds went to carbon and oxygen isotopes, then even 100 days would still seem to be a good amount of funds for the work proposed here. Similarly, carbon and oxygen isotope samples run about $16/sample, which would fund 5,000 isotope analyses. This is significantly more samples than proposed here. Again, these funds may be justified, but given the information here is does seem excessive.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	

	Overall, the work that the authors propose here, if successful, will provide very useful information on essential habitat for splittail, and could potentially provide critical data on the effectiveness of restoration efforts. The approach that is proposed is relatively new, but well accepted by the scientific community. Certainly it is a cost−effective technique for addressing these questions − indeed, in some applications (and this is perhaps one) it is the only technique that is likely to work. Having said that, it should be noted that the authors have limited experience with isotopic and elemental analyses, and therefore we have to take their abilities in this area on faith. I don't want to imply that the authors will not be able to conduct this research effectively. They may well be able to do it − they just haven't got a track record at this stage. Finally, although I applaud the use of carbon and oxygen isotope analyses that are planned, I think that they would do well to consider Sr isotopes as well in this system.
Rating	**good**

Technical Review #3

proposal title: Evaluation of the importance of typical nursery grounds of Sacramento splittail with otolith fingerprinting techniques

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Most of the goals of the project are worthy ones. Determine the habitat use patterns of splittail in the floodplain, estuary and adjacent waters of the central Valley. Goal 5 seems to make no sense. Juveniles don't spawn so I'm not sure what this goal is trying to accomplish.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The justification for the project is reasonable and the conceptual model is also a reasonable model for the project.**
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**In general their approach is reasonable if you choose to attack this problem with otolith chemistry. However I believe the project could be more creative in design. I would like to see some solution based ICPMS as part of this study to see how well solution based results match up with the laser ablation technique. The solution based approach can take individuals at different sizes and ages and the whole otolith comparison can easily be done with splittails down to late larval stages so the changes in the chemistry can be better defined by life history stages as well as habitat as a first cut to understanding the dynamics and pitfalls for using this technique with splittail. This will allow you to more quickly and cheaply define some of the chemical patterns by life stage (and through their life history) in the otoliths. Although this will not allow a time line for individual it will provide more precise chemistry and do it for much less. The other thing is that because otoliths come in pairs you can also do laser ablation on the same individuals, compare those temporal patterns and groundtruth your library. This will provide a better basis for judging temporal variability. Again costs would be reduced for the temporal if you only look at juveniles in a couple of habitats where they recruit with solution based techniques first. This would also allow you to select individuals for comparison that would yield the most productive results to meet the project goals.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**How can there not be temporal variability in the otolith signatures if the conditions in the Delta change from year to year and runoff and dilution of the chemistry changes? Of course there will be changes**

	and this is why I'm not sure the idea of an otolith library makes sense. You can best compare chemistry of the otoliths within a single season and among areas. Interannual comparison get dicey! There are a number of studies that show this and it makes sense because the chemistry of the system can change. You may also be able to distinguish natal areas for some individuals that rear in an area with an especially strong signature but estuaries and their chemistry are a continuum. There will be individuals that fall along the continuum. On page 7 in the methods they say they will transport the otoliths sealed in paper bags. I hope that is a mistake. Fibers in a paper bag could contaminate the surfaces of the otoliths. I'm guessing this is a typo of some kind.
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	
Rating	**not applicable**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**I believe this group is capable of doing the proposed research.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**I think the budget is a little high for what is essentially a single investigator proposal. Teh is basically overseeing his postdoc and technician and they will do all the work and probably write the papers and the reports. I know these kinds of analyses are pricey but 160K in machine time means you're basically buying 1/3 of an instrument. No estimate of cost of machine time per hour or per sample or estimated numbers of samples to be run are given so it difficult to be sure these costs are reasonable. These costs seem high compared to other labs that provide the same services. Also the travel budget seems padded for a local project and the office supply budget is padded. I've suggested ways this could be done more effectively and for less money.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**The PI states that this proposal is a cheap alternative to a tagging study. Otolith chemistry is a great tool especially for investigation of early life history where tagging is not possible. All too often it is a technique looking for a problem to solve rather than the best tool for the job. The proposed study may be a cheaper alternative to a tagging study but not a cheap one. However I'm sure a tagging study would provide more definitive results and perhaps at a cost not too much higher than 750K proposed here. Even if it costs twice as much, it would provide results that were not subject to statistical interpretation, probabilities or analytical errors like the otolith chemistry.**
Rating	**good**